IV. *Incidental observations in Pedigree* Moth-breeding.

By Frederic Merrifield, F.E.S.

[Read December 5th, 1888.]

*The experiments in hereditary moth-breeding which I reported last December are continuing. Of the broods there mentioned the forced *illunaria* have come to an end under circumstances I will describe; the sleeved *illunaria* are continuing in the A and M lines only, the Z line having failed, and the *illustraria* are continuing in all the three lines A, M, and Z. Without giving any detailed account of these experiments,—an account which will be more useful if deferred until they have been continued for some generations longer,—I propose on the present occasion to mention some facts, incidentally observed in the course of them, which appear to have a bearing on questions lately much discussed among naturalists, and may be of interest to members of the Society. I venture to hope also that my statements may elicit suggestions which will be of assistance in the further prosecution of these researches, and may thus perhaps prevent time from being wasted in pursuing them in directions not likely to yield valuable results, and save me from doing what more qualified observers have done before, or are doing much better than I can.

End of forced illunaria.—When my last paper was read on the 7th December, 1887, I exhibited some larvæ of the fifth generation of the year (reckoning the first generation as ending with the *moths* captured in the spring) of forced *illunaria*. At that time the larvæ numbered about 250. Only about thirty emerged as moths, and these appeared between 26th December and

* In some cases where the paper as read stated only general results, I have instead of these given full details, so as to enable others to judge as to the inferences to be drawn from them. These, with some additional explanatory remarks, are distinguished by brackets.

28th January. They showed unmistakable signs of deterioration, being for the first time smaller than their parents, and being poorer in colouring. All the eggs laid were sterile except one batch, of which 179 turned red. Thinking that the deterioration was probably, in part at least, attributable to the unnatural conditions in which they had been bred, which included continued forcing, and a food supply of winter leaves of evergreen-honeysuckle and rose, I decided to keep back the eggs, and consequently placed them out-of-doors from the beginning of February to the 14th April, a period during which the weather was cold, with much frost, and then forced them. On the 17th April they began to change colour, and in a few days all had turned black, and the young larvæ could be plainly seen through the shells of the eggs, but not one hatched. This experiment seems to show that long exposure to a winter temperature is not directly fatal to the eggs of this species, which under ordinary natural conditions are exposed only to a spring or summer temperature, and suggests the possibility that under changes of climate the insect, which now hibernates as a pupa, might come to hibernate as an egg. It also appears to indicate that the formation of the larva in the egg of this species is not gradual, but awaits the proper conditions, the chief among them being a sufficiently high temperature, and is then rapid. In this deferred development of the latest stage, the egg appears to bear some resemblance to the pupa, the development of which is referred to later on.

Illunaria, spring emergence, male larger than female.— It will perhaps be remembered that last year attention was called to the fact that though the female of *illunaria* in the summer emergence exceeds the male in wing-expansion, there was some reason to think that the reverse was true of the spring emergence. The latter opinion is confirmed by measurements since made of a great number of moths of the spring emergence. [In each of three broods, together numbering 86 males and 107 females, the average size of the male exceeded that of the female, the excess on the average of the whole being 0.26 mm., the largest male being 51 mm., and the largest female 49.50 mm. This compares with an excess on the part of the female in the summer emergence,—arrived at by measuring seven broods

numbering 244 males and 242 females,—ranging from 1.78 mm. to 4.00 mm. There was an excess in each of the seven broods, and the largest male was 41.93 mm., the largest female 44.68 mm. In *illustraria*, though there is a difference, it is not nearly so considerable; in each of six broods, three being of the spring and three of the summer emergence, the female exceeded the male in size. In the spring emergences, numbering more than 300 individuals, this excess on the part of the female ranged from 1.76 mm. to 2.30 mm., the largest male being 49.60 mm., the largest female 52.20 mm. In the summer emergences of more than 300, the average excess of the female ranged from 2.82 to 4.82 mm., the largest male being 39.50, the largest female 43 90 mm.] In brief, the spring female of *illunaria* is rather smaller than the male, the summer female considerably larger than the male; in both the spring and the summer emergences of *illustraria*, the female is considerably the larger, the difference being, however, less in the spring than in the summer emergence. The conjecture was thrown out last year that the same cause which tends to apterousness in the females of those of our Geometræ whose habit is to appear in the winter might be at work in reducing the wing-expansion of the female in one emergence only,—*viz.*, that which takes place at a cold time of year,—of a moth that has also a summer emergence. With regard to measurement I have found the expansion much more easy to measure accurately than the single-wing length is, and therefore, I now always measure the expansion. To convert the single wing measurement into expansion, it should be doubled, adding about 1-15th in *illustraria*, and 1-17th in *illunaria*. With reference to an inquiry made in my last paper, I may mention that the other English species of the genus *Selenia*, *lunaria*, rests in the position of *illustraria*, not of *illunaria*.

Influence of temperature on size, &c.—I hope the observations which follow may add to the existing materials for information on subjects that must often have seemed perplexing to those who have bred Lepidoptera on a considerable scale; for example, the differences between the spring and summer emergences of *illunaria* and *illustraria* in size and colour; why in some cases a pupa will yield almost immediately to a

high temperature, in others most reluctantly; how far the temperature affects the size, shape, markings, and colouring of the perfect insect, and in what particular period of development the change is caused. The well-known work of Prof. Weismann, as translated and annotated by Prof. Meldola, throws the greatest light on these questions, but cannot in the nature of things exhaust so large a subject. So far as my experiments on pupæ have gone, the results of them appear in close accordance with those of Prof. Weismann, but other experiments seem to indicate that the temperature to which the larva is exposed in its growing stages has much to do with the colouring of the perfect insect. Difference of size,—a kind of difference associated with seasonal dimorphism, may be supposed to depend on the conditions to which the larva in its growing stages is exposed, rather than on those to which the pupa is subjected. I think this difference of size in the *selenias* experimented on, as between the summer and spring emergences, not overstated at three to four as tested by weight, and five to six as tested by wing-expansion; but these proportions are by no means constant; and, besides the considerable difference in size between individuals of the same brood, there are also great differences between the same emergence, (*i.e.*, spring or summer) in different years.

I will first mention such results obtained as bear on the subject of size. It will be observed, on referring to the table given in my paper of last December, that the broods of *illunaria* forced at a temperature of 75° to 80° or upwards, increased in size from the "second" generation (first summer emergence) to the "third," and from the "third" to the "fourth" (they fell off in the next unhealthy generation); also, that of the three sets of the "second" generation, the sleeved were the smallest, the forced considerably larger, and the "bottled" somewhat larger still; *i.e.*, those which had been bred at an ordinary temperature were considerably smaller than the others, and of those others, such as had the higher temperature were slightly the smaller. 1887 was a very fine, dry, sunshiny summer in the south of England, and June and July were rather above the average in temperature, though all the other months were somewhat below. The summer of 1888 will be

fresh in the recollection of all as a very cold and wet one: in this year a brood of *illunaria* which I forced was larger, in a greater ratio than in last year, than a corresponding brood sleeved, the average expansion in the forced being 39.08, in the sleeved 38.16: the proportion of perfect moths emerging being also larger in the forced brood than in the sleeved brood. In the 1888 summer broods of *illustraria* the difference is in the same direction, but more marked. Though I did not measure the forced brood, the average size was obviously considerably larger than the corresponding sleeved brood. This sleeved brood, as compared with those sleeved last summer, is smaller and poorer in colour in all three lines, A, M, and Z, and has a larger proportion of cripples. I mention the proportion of perfect moths emerging in connection with size, because both seem greatly dependent on healthiness. In the autumn of 1887, out of 359 larvæ hatched out and sleeved, I obtained 317 pupæ, and bred from them 310 moths, of which but seven were cripples. In the summer of 1888, out of 415 pupæ of the sleeved summer emergence I bred 394 moths, but fully 87 of them were cripples. The case was a great deal worse with the autumn-feeding sleeved larvæ of 1888. Of 394 *illustraria* larvæ hatched out belonging to the A, M, and Z lines of descent, only 175 have pupated, and of these many are misshapen, and will die as pupæ or be cripples, and the average weight of the pupæ is less than 2-3rds that of the corresponding brood last year. Though I do not like to speak positively, I can think of no sufficient cause for this deterioration other than the extreme inclemency of the summer of 1888. The deterioration extends to some pupæ I have proceeding from eggs laid by a wild female taken in the New Forest last August; of 26 recently hatched larvæ sent me from this female in the second week of September, I have only ten in pupa, and the weight does not average much more than 2-3rds of those of last year; such superiority of weight as they show over the others being, perhaps, attributable to the circumstance, that owing to their backwardness I forced them for a time in their earlier stages. The pupæ in the M line of my sleeved *illustraria* are remarkably small, and these happen to have been sleeved on a tree in a particularly cold part

of my garden. During the very cold week at the beginning of October, my sleeved larvæ seemed to make no growth at all. On the 6th October I brought all indoors and forced them gently, and they had all spun up by the 29th. I may here remark that I shut up some *illustraria* larvæ with birch leaves that were quite sere and yellow, and they ate them; the "frass" was of a yellow-brown.

While speaking of the coldness of the summer, I may mention that Mr. Jenner of Lewes, to whom I gave some *illustraria* eggs in the spring, had fifty or sixty pupate in July, the first of them on the 5th, and the others slowly through the month. Two moths, a male and a female, appeared in July, the rest remaining over. I shall have something more to say of these presently.

One inference I should be inclined to draw from the foregoing observations is that the temperature most conducive to healthiness and large size in *illunaria* and *illustraria*, is one a little higher than that of a warm English summer. As to the cause of the usual difference in size between the spring and the summer emergence, I can at present suggest nothing beyond this, —that the larva of the spring emergence is much longer in feeding up than is the larva of the summer emergence, and I think I have generally observed that where there is no stunting or retarding from unhealthy conditions, those larvæ of a brood which are longest in feeding up are the largest.

As to the causes of variation in colour, markings, and shape, the inquiry is a more complicated one, of course embracing the pupal stage, though I am inclined to think, for reasons I will give, that it should by no means be confined to that stage. My experiments have been in two directions, an artificially high temperature for larva and pupa, and an artificially low one for the pupa, and are concerned with three species, *illunaria*, *illustraria*, and *Ennomos autumnaria* (the old *alniaria*). [In connection with this latter it should be remembered that it does not hibernate as a pupa, and is, I believe, everywhere a a single brooded species; I have seen no indication to the contrary after forcing it as larva and pupa, and then forcing for several months the eggs laid by the forced moths.]

Differences in appearance between spring and summer

emergences of illustraria.—This difference is easily seen, but is perhaps not so easily defined. Besides size and general look, the differences appear to be mainly these: —(1). The contrast between the outer edge of the dark transverse band and the light ground colour of the part of the wing outside it is generally much stronger in the spring emergence, and, indeed, does not usually exist in the summer emergence. (2). The costa is more rounded in the summer emergence, rarely becoming slightly concave in the outer half of the wing, as it often does in the spring emergence. (3). The wings are longer, narrower, and more pointed, and generally, more angulated in the spring emergence. (4). The outer edge of the dark transverse band on the fore wing of the spring emergence, before it curves, almost always forms an obtuse angle with the line of the costa beyond it; in the summer emergence almost always an acute or at most a right angle. (5). The same outer edge, where it approaches the inner edge of the fore wing, generally bends sharply towards the body in the summer emergence: slightly so, or not at all in the spring emergence. [(6). The transparent lunules are generally broader and more conspicuous, when the insect is held up to the light, in the spring than in the summer emergence. (7). The body in the spring emergence is more hairy.] Of all these I think Nos. 1 and 4 the most constant. It may here be observed that in both *illunaria* and *illustraria*, and in both emergences, the female is more angulated, and has more pointed wings than the male; this seems not unusual with those of our *Geometræ* which have any tendency to be angulated.

The variety in markings and colour, and also, I think, in size, I have found much greater in the summer than in the spring emergence [as Prof. Weismann leads us to expect that it would be]. I have endeavoured to increase these differences by selection, and with a beginning of success, as may be seen in the examples I have brought of the extreme forms yet obtained. Some are of a chestnut colour, with few markings above, and of a bright orange, with faint lilac markings beneath.

Effect of forcing on illunaria, illustraria, and autumnaria.—There is a difference of a general kind resulting from the forcing of the larva or pupa, or both. a

treatment which seems in all three species to produce a generally warmer and yellower hue, with a less amount of dark spots. This is to be noticed in *illunaria*. In the *illustraria* bred by me this effect is masked by the circumstance that the races I have belong mostly to two very different types—a warmly-tinted one with few markings, and a duskier one with conspicuous darker bands and patches. In *autumnaria* the difference is very conspicuous, as will be seen by comparing the forced brood, of which 26 have been preserved out of 29 bred (3 having been sacrificed for eggs) with the sleeved brood, of which 24 have been preserved out of 25 bred; and with the intermediate "bottled" brood: —(1), the general colouring of the forced is warmer; [(2), in all the spots of the forced contrast less strongly with the ground colour; (3), generally, if not always, the spots and marks of the forced ones are less dusky, and (4) not nearly so dark; (5), nearly all the males, and all but one of the females, have fewer spots than the corresponding sexes in the sleeved: on the under side the differences are more strongly marked.] The general result is that of the 26 forced there is only one that comes up to the general standard of the 24 sleeved in abundance and darkness of spotting and other marks, and there are only 3 of the sleeved which in lightness of spots and marks approach the general hue and appearance of the 26 forced.

[*Autumnaria*.—The difference in appearance between the forced and the sleeved being so marked, I give in some detail the facts, which appear to indicate that in this case the conditions to which the larvæ were subjected may have had much to do with the very striking difference in the moths. One of two conclusions at all events seems almost to follow from the experiments, *viz.*, that the larval period was the critical one, or that the colour of the perfect insect in this single-brooded and summer-pupating species can be affected by exposing the pupæ to a very moderate difference of temperature.

The eggs were kindly given to me by Dr. Chapman. There were 4 batches from (1), dark full-sized parents of British origin; (2), large-sized pale parents of unknown origin; (3), a cross between male (1) and female (2); and (4), a cross between male (2) and female (1). I took 10 from each of the 4 batches to make a batch of

40 for forcing at about 75°—80°, and similarly constituted batches of 40 for bottling and sleeving respectively.

Of the forced, 35 hatched from 21st to 29th May, and 29 emerged between 4th and 18th July, none cripples. The forcing began 21st May, when the first egg hatched, and of these I have a tolerably perfect record, having taken the spun-up ones almost every day from the time they began to spin—17th June—till they finished—1st July—and having recorded in the case of each pupa the date of spinning and of emergence. There were 15 males and 14 females. The larval period averaged 29·8 days, the range apparently being from 27 to 34 days, or more. The pupal period was as follows:—Males, average 16·37 days, range 13 to 18 days; females, average 15·32 days, range 14 to 17 days; average of both 15·86 days. (Pupal period with me commences with the spinning-up, owing to the difficulty of ascertaining when actual pupation begins with a larva that is enclosed in a cocoon. The actual pupal period is therefore several days shorter than that given by me; by 4 or 5 days in the sleeved pupa of *autumnaria*, and the larval period is lengthened in a corresponding degree).

The bottled were not so well attended to as the rest; whether for that reason or not, only 12 emerged, one of which was a cripple, out of 35 hatched between 21st May and 4th June. They were kept as larvæ on a table near the window, which generally stood open, and as they spun up were transferred to a tray at the other end of the room. About 24 spun up between 7th July and 10th August. The pupæ were forced from 12th August (at which time 3 had emerged), and the moths appeared from about 6th to 27th August, 7 males and 5 females, of which one was crippled. As nearly as I can get at them, the larval and pupal periods together averaged between 70 and 80 days, of which the pupal seems to have been about 30 in the males and 28 in the females; about 29½ in both sexes taken together.

Of the sleeved about 30 eggs hatched from 21st May to 4th June. During this period they were kept on the table above referred to. On the 5th they were transferred to a sleeve on a dwarf birch-tree in the garden, which is a cool one; 27 spun up from about 24th July,

to 29th August. The moths emerged from 17th August to 13th September, having been gently forced (about 65°—70°) from 6th September, after which date 7 or 8 of them emerged. As nearly as I can tell, the larval period averaged about 64, the pupal about 31 days. There were 16 males and 9 females, none crippled. The larval period can only be roughly estimated, as the spun-up ones were only taken when the sleeve was examined at considerable intervals. The first 5 were found 26th July, the next 18 on the 9th August, and as taken they were transferred to a tray indoors, which stood far from the window; I remember that on these occasions there were several that had only just begun to spin up. At this time (9th August) there were 5 larvæ feeding, and they were transferred to a bottle indoors; 2 of them spun up on the 10th August, and one each on the 25th and 29th August; these 4 were also transferred to the tray as they spun up.

The sleeved larvæ were therefore exposed during over 60 days on the average to the cold summer, the temperature of which, as taken about 8 a.m., averaged barely 57°, up to 9th August, when most had spun up. I do not think the temperature during this period ever reached 70° at that hour, and I have no doubt it was at night on one or two occasions near 40°. The temperature in the room where the pupæ stood, taken about 8 a.m., averaged about 66° during the time they were there, *i. e.*, from 24th July to 6th September (often higher, especially during the afternoon, when it must frequently have passed 70°), and rarely much lower. I should doubt if it ever fell below 58°, and that only on rare and brief occasions. I think it may therefore be taken that the pupæ of fully a quarter of the 25 moths which emerged passed their whole existence as pupæ at an average temperature of 66° or upwards, and that nearly all of them passed much the greater portion of their existence at that temperature. The larvæ and pupæ of those described as "bottled," the moths from which in colour and markings are not far short of the sleeved, were never at a lower temperature than that of the room, which, however, averaged several degrees lower for the 6 or 7 weeks preceding 24th July than afterwards.

If the great difference between the forced and the

sleeved originated in the pupal period, it would seem to follow that the difference between about 66° and about 75°—80° during an exposure of a very few weeks is sufficient in this species for the purpose. It has been suggested to me by Professor Weismann's remarks (pp. 73—5) on the insufficiency of a difference of 14·9° R. (33·5° Fahr.) between the German winter and summer temperatures to originate a climatic variety of *P. podalirius*, while a difference of about 4° R. (= 9° Fahr.) between the summer temperatures of Germany and Sicily is sufficient, and his inference that the cause is to be found in the absolute temperature reached, that the explanation of the difference between the forced and the sleeved *autumnaria* is to be sought rather in the upper than in the lower part of the thermometric range, *i. e.*, in the high temperature to which the forced were exposed, a temperature which, as it happens, closely approximates to the summer temperature of Palermo, 19·4° R. (75·6° Fahr.), rather than the low temperature to which the sleeved were subjected.]

Effect of cold on pupæ of summer emergence. — The observations made as to the effect of cold on the pupa are as follows :—I begin with the effect of icing on pupæ that would in due course emerge in summer. I had 174 sleeved *illustraria* A pupæ, which had spun up from the 4th to the 15th July. All but 39 were forced from the 17th. Nearly all of these emerged, and they did so from the 20th to the 25th July. The 39 (23 males and 16 females), taken indiscriminately, except that large ones were passed over so as not to prejudice the heredity experiments, were on the 17th July exposed to a steady temperature of 33°—34°, and so kept until 1st August, *i. e.*, for 15 days, when they were forced, and all emerged in the 3 days from the 4th to the 7th, except one, which appeared on the 9th. As a whole they are noticeably, but not strongly, darker in hue than the others ; in the females especially there is a tendency to the contrast between the dark inner and light outer portion of the wings, which is so marked a feature in the spring emergence.

The effect of icing on the rate of development of pupæ preparing for summer emergence appears, so far as my observations have hitherto gone, to be to arrest [or rather to retard] the development so long as the icing is

applied and so long only, it being taken up again sharply the moment the change is made from the low temperature to the higher one. [I add some detail on this point, as it seems to indicate either that extreme cold as applied to the pupæ of the summer emergence does not absolutely arrest the development during the period of its application, but allows it to go on slowly so as to shorten the rest of the period, or else causes the development to be more rapid after the icing ceases. The first experiments, of which I have a full record, were with *illunaria*, and the low temperature in this case ranged only from about 40° to 44°, the pupæ not being close to the ice. On the 7th June I took for icing 11 males which had spun up at known dates from 30th May to 7th June, and 3 females which had spun up at known dates between 1st and 7th June. On the 16th June I added to them 6 males and 9 females which had spun up at known dates from 4th to 8th June. All were of the same brood, and had been forced up to the time of removing them to the ice-box. The cooling in this case did not produce any change of colour, or, if any, it is a very slight one. As removed from the ice-box they were replaced in the forcing-box. As compared with those which had remained there all the time, the average period in pupa, exclusive of the days in the ice-box, was as follows :—

	No.	Not cooled. days.	No.	Cooled 7 days. days.	No.	Cooled 16 days. days.
Males ..	19	12·500	6	11·666	11	9·682
Females ..	13	12·192	9	10·833	3	8·500

The record as to the *illustraria* is not so fully kept. These were iced 15 days, and kept at a steady temperature of 33°—34°, *the moths, as a result, being noticeably darker in hue*. I can only get a general average of the time of spinning up, which I assume to be the same for iced as uniced, the former having been taken promiscuously; and, though I have a record of the uniced that emerged next day, my only record as to the iced is that all emerged from the 4th to the 7th August, except 2, which appeared on the 9th. The figures therefore are worth but little; such as they are, they incline in the same direction as those of the *illunaria*, but in a much slighter degree.

	No.	Not iced. days.	No.	Iced. days.
Males	73	13·042	23	12·652
Females ..	64	13·031	16	12·719

The apparently more rapid development of the female than of the male in *illunaria* (also, as will have been noticed, in *autumnaria*) was unexpected, and probably the experiment has not been on a sufficiently large scale for this apparent result to be relied on. Where there is a difference in priority of emergence of the sexes, I have usually found it rather in favour of the male, so that if the female develops more rapidly in the pupal stage, that would perhaps involve a slower development of that sex in the earlier stages.]

Effect of cold on pupæ of spring emergence.—[I should premise that forcing these, except when it was deferred until they had been some months in pupa, produced very unhealthy results, as shown by the small percentage of survivors and the great proportion of cripples.]

The observations which immediately follow have relation to the effect of temperature on those pupæ which would in due course emerge in spring. In 1887 my second forced brood of mixed *illustraria* (offspring of about 10 pairs), all hatched between 4th and 10th August, and all treated exactly alike, instead of all feeding up, as the *illunaria* had done, split into two divisions; the first, consisting of 28, pupated rapidly between the 22nd and 30th August, and all emerged in perfect condition between 30th Aug. and 8th September, *i. e.*, in from 8 to 9 days. Of the rest, a few died as feeding larvæ, more in pupating, which began 9th Sept., 10 days after the others had left off; but the majority of them became pupæ, and would no doubt, under natural conditions, have remained over until next spring. [This splitting up of a brood into two seems plainly to have been caused by an artificially heightened temperature; in other words, subjecting the *larva* to a heightened temperature caused a portion of the brood, which would otherwise have remained over and appeared as moths in the spring and with the spring colouring, to appear as moths in the same summer and with the summer colouring; *i. e.*, the decisive point was in this case reached in the larval stage, and reached by the application of a high temperature. I am aware that Prof.

Weismann's experiments show the pupal stage to be one specially affected by temperature. The subject is too considerable to be discussed here, but I venture to point out that two features associated with seasonal dimorphism, *viz.*, the important one of size, and, as it would appear from Mr. W. H. Edwards' experiments, the very important one of shape, are determined in the larval stage ; and that in the three species experimented on by me the *larval stage was in fact much shortened* by the high temperature applied (see this paper, *passim*, also Trans. 1888, pp. 130, 131, 132—3); whereas in those experimented on by Prof. Weismann the larval period is stated to have been very little shorter in one brood than in the other, so that the duration would not,—and, it is indeed stated, did not,—engage attention (Weismann, by Meldola, p. 10).] Though kept at a forcing temperature, none emerged until 11th November, a period of more than 2 months; a second appeared on the 12th November ; and 5 more appeared between the 9th and 26th December. Six of the seven were cripples, and all the 7 were of the summer type, but duller in colour, and the latest of the 7 are no darker than the earliest. On 7th November, after gradually cooling them down for a fortnight, I put 10 pupæ out-of-doors, where they had a good deal of frost before they were brought indoors on 1st January, a period of 9 weeks. One had come out in the interval, and another appeared 3rd January. From the 9th they were forced, and 4 more appeared between 22nd January and 28th February. All 6 were distinctly of the dark spring type, though rather poor in colouring, and 3 were cripples. In this case a marked effect seems to have been produced on the colouring of the moth by exposing the pupæ to a freezing temperature, [and no such effect produced by considerable retardation without cooling.]

I mentioned some pupæ which Mr. Jenner had, and which, with the exception of 2, would evidently have been single-brooded had they not been subjected to artificial treatment. He kindly lent half of them, consisting of 10 males and 16 females, to me, and on 10th September, 1888, I placed them in a forcing-box, which was kept at 75°—80°, except for the first fortnight in October, when, for reasons connected with another experiment, it stood at 60°—65°. Nine emerged, all

females, on the following dates: Sept. 27 (2), 30, Oct. 3, 7, 8, 14, 16, 19. The one which emerged 8th October was accidentally destroyed, the remaining 8 (exhibited) are of the same general colouring, which seems to be intermediate between the spring and summer colouring, and differs most materially from that of the 2 which emerged in July. The later ones of the 8 are much darker than the earlier ones, and, though the progressive darkening is not quite regular, it is difficult to look at them without thinking that retardation of development has been the operative cause, the retardation in this case not being associated with cold, as the pupæ have rarely or never been at a lower temperature than 60° [There are some other facts which rather favour the view that retardation without cold may be a cause of, or at least be associated with, darkening. Thus the numerous specimens I have preserved of *illunaria* show that where one or more of a brood have emerged several days after the rest they are often in a marked degree different in colour, generally darker. And a sleeved female *illustraria* of the summer emergence of 1888 before mentioned, which was rather behind the others in feeding up and was not forced at the end, and which, instead of corresponding with the average period of about 13½ days (of which 3½ forced) remained from 20 to 22 days in pupa, makes as near an approach in general hue to the spring emergence as do the iced ones of that brood.]

Simultaneously with the offspring of the forced *illustraria* of the 1887 summer emergence, I brought up some 29 in a bottle at the ordinary indoor temperature, and the pupæ remained at the same. They showed no tendency to split up, but, of the 20 that spun up, all did so between 12th September and 5th October. Ten were on 27th November put out-of-doors, and were brought in 1st January, and, after being gradually warmed, were on 9th January placed in the forcing-box, and kept there at 65°—70°. These straggled greatly in their emergence, *viz.*, Jan. 28, Feb. 13, 14, 15, 16 (2), 17, 23 (2), and 28; 5 others, without being frosted, were on 29th January placed in the forcing-box; these emerged Feb. 9 (3) and Feb. 11 (2); 5 others, not frosted, were on 3rd March placed in the forcing-box, and they emerged March 9 (1), 10 (1), and 11 (3). All the 20 were perfectly healthy specimens. Comparing the first 10 with the

second 5, it looks as if the frosting in this case had the specific effect of causing the subsequent partial retardation and great irregularity in emergence, an irregularity represented by a range of from 19 to 50 days, though the facts as to the other 2 lots of 5 each which emerged respectively in 11—13 days and 6—8 days seem to show that the earliness of the date at which the forcing began had something to do with this. All are of the spring type, and there is no marked difference between those which have been frosted and the rest. I have reason to think the unfrosted pupæ were never at a lower temperature than 40°, which therefore, if and so far as lowness of temperature is a cause of this dark colouring, appears to be low enough for the purpose in *illustraria*.

Effects of forcing in different cases.—It seems to be an interesting question what are the causes which determine whether the pupæ of a double-brooded moth shall fall into the category of those that will emerge in spring, or into the category of those that will emerge in summer. There is of course a very real distinction between the two categories, one obstinately resisting for months a high temperature to which the other succumbs in a few days; and part of a brood will range itself in one category, and the rest of it, though subjected to exactly the same conditions, in the other. The different behaviour, with me, of the two species will be noticed, *illunaria* yielding several forced broods in succession, while *illustraria* gave one and part only of the next. The case of *illunaria* proves, if proof were needed, that it is not a case of alternative succession of the two different forms, while the instance given of the forced *illustraria* splitting into two shows that some other cause than a difference of temperature must be concerned in occasioning it. Though temperature appears to have so little effect on a pupa that is not ripe for it, the effect of it on a pupa which is ripe for its operation seems immediate and tolerably regular, both with the spring and the summer emergence. [I give a summary of the effect of forcing on the pupæ of some considerable broods :—

Sleeved Illunaria.

Spring emergence (3 broods).

Apr. 16. Forcing began	♂	♀	Total.
,, 18. Emergence of	0	+ 1	= 1
,, 19. ,,	27	+ 31	= 58
,, 20. ,,	56	+ 53	= 109
,, 21. ,,	13	+ 26	= 39
,, 22. ,,	1	+ 8	= 9
Individuals	97	+ 119	= 216

Summer emergence (2 broods).

July 17. Forcing began	♂	♀	Total.
,, 17. Emergence of	0	+ 1	= 1
,, 18. ,,	4	+ 9	= 13
,, 19. ,,	18	+ 24	= 42
,, 20. ,,	28	+ 20	= 48
,, 21. ,,	11	+ 11	= 22
,, 22. ,,	8	+ 2	= 10
,, 23. ,,	1	+ 2	= 3
,, 24. ,,	0	+ 2	= 2
Individuals	70	+ 71	= 141

Sleeved Illustraria.

Spring emergence (3 broods).

May 10. Forcing began	♂	♀	Total.
,, 11. Emergence of	1	+ 0	= 1
,, 12. ,,	59	+ 6	= 65
,, 13. ,,	94	+ 97	= 191
,, 14. ,,	6	+ 46	= 52
,, 15. ,,	0	+ 1	= 1
Individuals	160	+ 150	= 310

Summer emergence (3 broods).

July 17. Forcing began	♂	♀	Total.
,, 20. Emergence of	3	+ 3	= 6
,, 21 ,,	56	+ 31	= 87
,, 22 ,,	96	+ 83	= 179
,, 23 ,,	52	+ 38	= 90
,, 24 ,,	10	+ 9	= 19
,, 25 ,,	0	+ 3	= 3
Aug. 4—6 (not forced).....		1	= 1
Individuals	217	+ 168	= 385

It will be observed in the case of the spring emergence that when the winter was well over and the pupæ may have been supposed to be ripe for emergence on suitable conditions presenting themselves, the *illunaria* nearly all emerged in from 3 to 5 days after the forcing was applied, more than half of them on the 4th day, and the *illustraria* nearly all emerged in from 2 to 4 days after the forcing was applied, more than half of them on the 3rd day.] It seems as if under the most favourable conditions it took several days to go through the changes that intervene between the comparatively torpid central period of pupal existence and the winged stage, and that, as the icing experiments appear to indicate, a "cold snap" occurring during any part of this active later period may suspend these changes for a time, but produce no great effect besides. I suppose there is a

point in the life of a pupa when it may be said that a corner is turned, and that the last stage of development has begun, a point that seems indicated in *E. versicolora* by the pupa's forcing itself out of its cocoon, the pupæ that do not thus come up remaining over till another year. It is, I believe, a common observation in collecting that it takes several warm days in succession to bring out the moths that emerge in spring, and that after such a succession they often become rather suddenly plentiful. A passing remark on the usefulness of the forcing-box will not be out of place here; with some experience it should enable one who is breeding insects to bring them out almost to the day [if in combination with a sufficiently capacious ice-chest] at the time when he may be able to attend to them; without such appliances the heredity experiments as to size would be rendered much more difficult, as in warm weather a considerable percentage of moths, if kept a few days, though in the dark, flutter so as to damage their wing-tips, and make it impossible to measure them.

One general result of the experiments appears to be that cold applied in the earlier stages (there being strong indications that the larval period is one) in the life of the 3 species experimented on, 2 of these species being double-brooded, and one single-brooded, has a tendency, operating possibly by retardation, to produce or develop a darker hue in the perfect insect: if so, it may perhaps throw some light on the melanism so often remarked in north-country examples of widely-distributed moths.

In conclusion, I hope to be allowed to say that I feel as strongly as any one can that the record of my experiments would have been more satisfactory to others, as it certainly would have been to me, had it been deferred until they could have been brought nearer completion. But I trust the reason which decided me to offer them in this imperfect form may be thought a sufficient excuse, the reason being that I hoped others might be induced to follow them up in the various directions in which they point, and that, as to such as I may be able to prosecute, I may receive suggestions enabling me to conduct them the better. I have a great many recorded observations other than those I have made use of here, and they are entirely at the service of any who are interested in the subject; they give, in a tabular form,

the parentage, birth, and life-history of considerably more than a thousand individual moths, which have been preserved. I have to thank the Editors of the 'Entomologist's Monthly Magazine' and the 'Entomologist' for their help in drawing attention to my desire to obtain living examples of *illustraria* from other regions than the South and East of England (especially from regions where it is single-brooded, as I believe it is in Perthshire and in Scandinavia), as materials for the inquiries I am desirous of prosecuting; this assistance has not yet borne fruit, but the insect is not a common one, and I hope it may some day find its way to me from these distant parts.